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chemicals, so that the condition under which entelechy comes into play is always realized. We may therefore expect its action at any step in our work; we must be prepared at all times to find the same physical configuration giving rise now to one result, now to another; we can have no confidence that when two experiments give different results, it will be possible to find an experimental cause for this difference.

Doubtless there are investigators who can persuade themselves that they really believe this sort of thing, and yet who can continue hopefully their hopeless task of trying to discover experimentally the conditions that determine what happens—just as there are persons who assert that they believe certain orthodox religious doctrines and yet live cheerfully the life of the worldly. But for one who takes his experimental work seriously and who has use for theories only as *theories of practise*, the acceptance of such a doctrine can not fail to profoundly change his work and his attitude toward his work.¹⁴ It takes away the guiding principle on which every step of his work is based.

Thus a doctrine which holds to consistent physical determinism in the inorganic sciences and rejects it for biology makes a tremendous difference in principle between the two fields; a difference big with practical results. I believe that to most working investigators of biology the question of vitalism means the question whether there is such a difference, and it appears unfortunate that

¹⁴ Of course there would still be work for the biologist. Descriptive and observational work would be little affected. The biologist could substitute "entelechy" for "god" or "providence" or "nature" in the pious expositions of the naturalists of two generations ago, and devote himself to showing the wonderful and unfathomable ways of entelechy. If of an incurably analytic turn of mind he could even examine the limitations which the physical conditions place upon entelechy: and perhaps make a catalogue and classification of the various results produced by entelechy from a given physical configuration. It is the principles, methods and objects of experimentation that would be changed.

this question should be obscured by confusing it with the (for the working investigator) relatively inconsequential question as to whether anything happens in living things that doesn't happen in those not alive.

H. S. JENNINGS

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May 16, 1911

THE APPLICATION OF THE METHOD OF LEAST SQUARES

TO THE EDITOR OF SCIENCE: It would, I think, be interesting and valuable to have a consensus of opinion from both astronomers and physicists as to the limits within which the application of the method of least squares is permissible. This method is used widely by astronomers and but rarely by physicists. Moreover, I believe most physicists would hesitate to push the application of the method as far as is commonly done by astronomers.

To take a concrete case: During the discussion that followed the Saturday afternoon symposium at the recent general meeting of the American Philosophical Society, one point under discussion was whether or not the principle of relativity requires the abandonment of the concept of the ether. The writer mentioned as an *experimentum crucis* the possibility of detecting an ether-wind by measuring the speed of light in a single direction and over a path which for its greater part lay remote from the surface of the earth, thus avoiding a limitation of the Michelson-Morley experiment. It was suggested that if the measurement of the speed of light by Römer's method could be carried out with sufficient accuracy, and at two such times that the light would have to travel with and against the proper motion of the solar system, such an ether-wind might be observed. It was pointed out that the difference of time to be expected would be of the order of one fifteenth of a second. Some doubt was expressed as to whether this accuracy was yet attainable in a difficult measurement of this nature.

To this Professor Pickering replied that a large mass of such data was already in the possession of the Harvard Observatory, and had been discussed and reduced with this very

point in mind, and with a negative result. In response to a query from Professor Doolittle as to the precision of the observations Professor Pickering stated from recollection that the error of a single observation might be three or four seconds, but that the hundreds of observations available brought the probable error well within the precision required.

It seems to me there is room for an honest difference of opinion as to the value of the method of least squares in a case like this, where the error of a single observation may be forty-five or more times as great as the quantity to be detected. Surely we must draw a line somewhere.

While I have given this point no extended investigation, I may formulate my own opinion, as a basis for discussion, in the form of a mathematical theorem:

The value of the measure of precision obtained by applying the method of least squares varies inversely as the ratio e/q (where e = error of a single observation and q = quantity to be measured), in such a manner that when $e/q = 1$ the value is zero, and for $e/q > 1$ the value is wholly imaginary.

PAUL R. HEYL

DR. BRUSH'S THEORY OF GRAVITATION

To THE EDITOR OF SCIENCE: The article by Dr. Brush on "A Kinetic Theory of Gravitation" in SCIENCE of March 10, will become of great interest to physicists when the author does what he partly promises to do in a future paper, viz., explains how a body which is perfectly transparent to a given radiation can shield another body from that radiation, and why, if the other body is also perfectly transparent, it makes any difference whether it is shielded from the radiation or not. It would appear to be immaterial, so far as the effect upon the body is concerned, whether the atoms of a body through which this radiation is streaming in all directions are "buffeted about in every direction by the ether waves in which they are entangled" or whether they remain undisturbed by these waves, so long as they do not absorb any energy from the radiation.

Dr. Brush says that in the former case, "Each atom or molecule may be regarded as a center of activity due to its kinetic energy of translation, with continual absorption and restitution of the ether's energy normally equal in amount." This seems to the present writer equivalent to saying that a perfectly transparent body may be regarded as one in which the atoms are continually absorbing and radiating equal quantities of the same kind of energy. If anything can be gained by making such an assumption, there seems to be no objection to making it, and I, for one, shall look forward with interest to Dr. Brush's explanation of how it will enable such a transparent body to cast a shadow.

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SCIENTIFIC BOOKS

Soil Fertility and Permanent Agriculture.
By CYRIL G. HOPKINS, University of Illinois. Pp. xxiii + 653; 14 illustrations, 3 colored maps. Boston, New York and London, Ginn & Co. Price \$2.50.

Of this work the author says: "The chief purpose of this volume is to bring together in convenient form the world's most essential facts, gathered from the field and laboratory, and to develop from them some foundation principles of permanent agriculture." The book is a notable contribution to the foundations of practical agriculture, treated in an introduction and four parts, I., Science and Soil; II., Systems of Permanent Agriculture; III., Soil Investigations by Culture Experiments; IV., Various Fertility Factors.

The method of treatment adopted is admirable but not that usually chosen by writers on either soil or agricultural chemistry. The book takes a distinct place in agricultural literature and will be found a mine of information and valuable reference to the subjects it treats. Professor Hopkins holds persistently throughout the volume to the thesis named in the title and does not aim to treat in detail a wide range of topics, but has built his treatment on a broad, most substantial